a trace of the appearances proper to *rabies canina*. Zimmerman says, that in all known cases the disease found its origin only in the bite of a dog already mad, and that other animals, as horses, so bitten, likewise became mad. The last opinion seems to require confirmation; the former is not borne out by his facts of fifteen dogs bitten by a known mad one, two only became mad; two ran away, eleven remained healthy.—*Monthly Journ. Med. Sci.* Oct. 1854.

32. *Cutaneous Anæsthesia in Lunatics.*—A lunatic received a severe injury of the great toe by the fall of a heavy piece of wood, so that the nail was torn away. Dr. Snell, on examining the patient, remarked that he seemed scarcely to feel this injury, ordinarily so very painful. On examining the case more minutely, he found that this lunatic had completely lost cutaneous sensibility. This circumstance led him to examine all the lunatics in his establishment, and he found that out of 180 insane persons (100 males and 80 females), 18 (17 males and 1 female) had complete anæsthesia of the skin; and that in 6 others, who were males, the sensibility to pain was very much decreased.—*Monthly Journ. Med. Sci.* Oct. 1854, from *Zeitschrift für Psychiatrie*, x. 2.

33. *Lumbrici in the Left Pleura.*—Lumbrici have been found in many parts of the abdomen, and even free in the peritoneal sac; but Professor Luschka communicates an extraordinary case, in which, through the intermediate process of a retro-peritoneal abscess, four lumbrici were found encysted in the left pleura.

A man, æt. 23, who two years before had had slight peritonitis, suffered, in 1852, from return of this complaint, with especial pain in the left lumbar region; death ensued, with typhoid symptoms. In the left pleura, between the lower lobe of the lung, the thoracic wall, and the diaphragm, there was a sac formed of pseudo membrane, in which six lumbrici and a large quantity of brown fluid were contained. An opening in the diaphragm led into a cavity formed by adhesions between the upper end of the descending colon, the left kidney, and the diaphragm, and in which some lumbrici were also contained; this cavity, or abscess, communicated with the descending colon by three contiguous openings, situated on a level with the under part of the spleen. In the colon there were also lumbrici.—*Brit. and For. Med.-Chirurg. Rev.* from *Archiv. für Pathol. Anat.* bd. vi. heft 3.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

34. *Curability of Cancer, and its Diagnosis by Means of the Microscope.*—At a meeting of the Academy of Medicine, held on the 26th September, M. Jobert presented a report on a case of sarcocele in a young infant, in which M. Pamard, of Avignon, operated, and *cured* the little patient. The alleged *cure* in this case gave rise to an interesting discussion, which is not yet concluded, on the curability of cancer, and its diagnosis by means of the microscope.

M. Robert doubted if encephaloid cancers were ever cured. The little patient had, doubtless, recovered from the disease, but it was not exempt from its recurrence.

M. Velpeau moved that the question of cancer should be brought before the Academy at an early meeting. All surgeons were not of M. Robert's opinion as to the incurability of cancer. He expressed doubts as to the value of the microscope in the diagnosis of cancer, and undertook to prove its fallibility in the diagnosis and prognosis of cancerous tumours.

October 3d.—On the President announcing that this was the day appointed for the discussion of cancer, much amusement was created by M. Velpeau declining to speak before M. Robert, and M. Robert refusing to speak before M. Velpeau.

M. Leblanc opened the debate on the curability or incurability of cancer, by briefly going over the researches which he had made on the subject with M. Trousseau, and those he had lately made microscopically, conjointly with MM. Lebert and Follin. He considered the microscope of great service for the diagnosis of the cancers so common among animals. He did not believe in the incurability of cancer; and considered that an *early* operation diminished the chance of recurrence. His speech contained much valuable matter, but no new facts.

M. Barth followed, making, on this occasion, his *début* in the Academy. His address was a very excellent one. As to *diagnosis*, he considered that, in a very great number of cases, this may be made with the naked eye, but that sometimes the characters were better seen by means of the microscope. In these first cases, the microscope afforded additional certainty of diagnosis. In a certain number of cases the physical characters are doubtful, and in these the microscope furnished an essential element of diagnosis; but it did not always remove doubts, and might sometimes even occasion mistakes. As to the *curability* of cancer, he thought that absolute incurability was disproved by facts, and unsupported by analogy. We should take into account all the characters of the tumour—those inherent in it and accessory to it—in order to determine the possibility of chances of recurrence.

M. Gerdy made some very interesting and novel observations on the anatomico-pathological characters of the sanguineous cancerous tumours. It was singular that, although these tumours were known in science, there was nothing said about them in books. How were they to be distinguished? Chiefly by their anatomical characters. They were rich in bloodvessels, and in *splenoid* tissue analogous to the placenta. Surgeons know that they recur after extirpation. Some of these tumours were situated in or near the bones, and were vascular and pulsatile, analogous to the spleen or placenta. Breschet, Dupuytren, and Lallemand, called them simple tumours, although they have found colloid matter in them; but they are not such. He considered the microscope useful in many cases, but in others as likely to confuse.

October 10*th*.—M. Robert: I have said that the cure of cancer, especially of the encephaloid variety, is an exceedingly rare fact, if it can be well proved. It seems that M. Velpeau is not of this opinion. On consulting his work on diseases of the breast, I find that he has operated in 250 cases, in which the patients have survived the operations. He lost sight of 100 of these patients after one year, 150 after two years, and 200 after five years. It is matter of regret that, in these cases, the final result was not ascertained. Of the 50[1] remaining, 20 have been free from the disease for five, ten, fifteen, twenty, and even twenty-five years; and these cases M. Velpeau regards as radical cures. This enormous proportion of cures seems astonishing, when we call to mind the results recorded by other eminent surgeons. Munro says that 4 out of 60 cases in which he operated for cancer were free from the disease for two years; but that three of them eventually had occult cancer of the mamma, and the fourth an ulcerated cancer of the lip. *Ergo*, 60 *operations and* 60 *relapses*. Mayo says that carcinoma of the mamma, even when operated on under the most favourable circumstances, and removed early, recurs in 95 *cases out of every* 100. Macfarlane operated in 32 cases of cancer, and collected the particulars of 86 other cases of his colleagues; and out of these 118 cases *there was not one in which the malady did not recur*. Boyer says that out of more than 100 cases in which he has known individuals operated upon for cancerous tumours of the breast, or of other parts, he has only known *four or five cases* which were thereby radically cured. M. Broca, in his prize work on cancer, says, that of 19 cases in which tumours were removed by Blandin, which were demonstrated microscopically to be cancerous, *not one* remained exempt from a recurrence of the disease for two years. Lebert says that, of 34 cases operated on for cancer, 6 died after the operation, 21 had a recurrence of the disease within a period varying from three months to two years; and the remaining 7 were lost sight of. I have said enough to show how unanimous are the opinions of the greatest authors as to the curability of cancer.

I cannot help thinking that the marvellous discrepancy in M. Velpeau's statistics has been due to errors of diagnosis, and to his having mistaken benign for malignant tumours. The surprising cases which he says remained cured for fifteen, twenty, or twenty-five years, must have been seen at a time when an error in diagnosis would have been quite excusable even in M. Velpeau. But M. Velpeau says he has never been deceived; and that in more than a hundred cases his diagnosis has been confirmed by the microscope. This, I would remark, *en passant*, is not very flattering testimony in favour of the microscope. Let me recall to your recollection the discussion that occurred in 1844, in the Academy, on fibrous tumours of the mamma. On that occasion, M. Cruveilhier described very excellently the anatomical and clinical characters of those non-malignant tumours of the breast, which were often confounded with cancer, and showed how he had often dispensed with operations by recognizing their presence. His opinions, and above all, the name of "fibrous bodies," which he gave these tumours, were combated by MM. Bérard, Blandin, Roux, Amussat, etc., who admitted the existence of such benign tumours, but held that it was impossible to distinguish them from cancer. M. Velpeau, on that occasion, defended the opinion of M. Cruveilhier. That he has studied these non-malignant mammary tumours, is evident from a paragraph in his article MAMELLES, in the *Dict. de Méd.*, in thirty vols. He affirms that diagnosis was possible.

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[1] [There is evidently some error in these figures.—Ed. Am. Jl.]

But he had not *then* the experience which enabled him *now* to rectify error in the matter. Hear, therefore, how he then expressed himself before the Academy: "M. Cruveilhier has gone too far in affirming that fibrous tumours of the breast may always be easily distinguished from those which are cancerous. In some cases this distinction is very difficult, and even impossible." And he further added: "The defiance which MM. Bérard, Blandin, and Roux have given M. Cruveilhier to recognize on the living woman a benign tumour, I would not be afraid to accept myself; and I do not hesitate to say, that if *I only committed four errors out of ten cases, I would feel quite satisfied.*" Gentlemen, may we not with justice conclude, after this avowal, that among M. Velpeau's cases of cancer, cured before the year 1844, there must have been several patients who were operated on for merely non-malignant tumours? And observe that, by a singular coincidence, the proportion of four errors of diagnosis out of ten is precisely that of the successful cases in his statistics, viz. 20 in 50.

Since the year 1844, the study of benign tumours of the breast has greatly advanced. Velpeau erred at first in calling them fibrinous; and it was only after the researches of Lebert that he gave them the name they still retain, *adenoid* tumours. These researches have greatly elucidated the pathology of tumours, and thus advanced clinical medicine; while the microscope, in revealing their minute structure, has made us acquainted with physical characters never before observed. I cannot admit the infallibility which M. Velpeau claims for himself, for I believe that cases are still met with which may deceive the most able practitioner. It must, therefore, be believed that M. Velpeau's results are not rigorously exact, or capable of proving that the cure of cancer is less rare than is supposed.

Hitherto, we have only been discussing the two kinds of mammary tumours which are liable to be confounded with each other; but if we turn our attention to the tumours of other organs, we will find the causes of the errors of diagnosis to be more numerous, and hence we can understand thereby the diversity of opinion which exists among surgeons as to the curability of cancer. In these organs, besides cancer and hypertrophic tumours analogous to those of the breast, we find, also, epithelial and fibro-plastic growths. The latter, however, present some affinity to true cancer, and their external characters do not permit of their always being clinically recognized; consequently, they may grow again after the operation. Now, persevering microscopical researches, made by eminently qualified and conscientious men, have demonstrated, on the one hand, that the minute structure of such tumours differs essentially from that of cancer, and that often the naked eye may distinguish them from it by incontestable characters. Clinical observation, on the other hand, has shown that, if these growths reappear after extirpation, they do so less frequently than cancer; that the relapses are generally local, and admit of fresh operations being performed with a better chance of definite success; and, finally, that their becoming generalized, or causing death, only occurs in rare cases, and is exceptional.

If we would put an end to the confusion which exists at present on all that relates to cancer, and above all to its curability, we would sedulously exclude from the question all those epithelial and fibro-plastic growths which have only special analogies to it, but which differ essentially from it in their intimate structure and evolution.

M. Velpeau: M. Robert, in his speech, has only discussed one side of the question, viz. the curability of cancer; and he has said nothing of the value of its microscopic characters, although he appears to be a partisan of the microscope. He complains that I have not given the definite results in a large number of my cases of operations. Among the 200 patients whom I unavoidably lost sight of, relapses occurred in a certain number of cases, but in the rest I never could ascertain the results. Out of the 50, whom I carefully watched, there are 20, all of whom I could find to-day, perfectly cured. M. Robert explains this considerable proportion of cures by premising errors of diagnosis. I do not pretend to be more infallible than him; I have sometimes been deceived; but there *are* cases in which one *cannot* be deceived. I have taken great care to call *cancer* only those cases which presented all the characters with which it was impossible to be deceived at the bedside. In these cases, the microscope has never shown my diagnosis to be fallacious; and the same has occurred in every case in which I diagnosed an adenoid tumour. Far from undervaluing microscopical researches, I have always encouraged them, for I wish much to see science possessed of more certain marks than at present. But in granting a high place to the microscope for diagnosis, clinical observation must not be excluded. When M. Robert meets an ulcerated, fungous, softened tumour, he cannot be deceived; nor can he, if he meets one which is hard, knobby, and uneven. For my own part, I confess that in such cases I would not be deceived; and that the microscope has always confirmed my opinion, I would appeal to M. Lebert, who has for a long while attended my visits at *La Charité*.

M. Robert, in recalling the former Academy discussion of 1844, wishes to make me contradict myself, in making me say that I had then confessed having been deceived. I still frankly admit that there are cases in which I may be deceived. When I say that I have always diagnosed the tumour, I mean in the cases in which they presented their habitual characters.

I repeat, I have been far from discouraging microscopic researches. For thirty years I have studied the subject of cancer with lively interest, and a sincere desire to arrive at the truth. In 1836, I stimulated M. Donné to make researches in this direction, and later still, I advised M. Lebert to follow out the inquiry. I said to them: "Search; for cancer ought to have a special character." I was especially anxious that something might be found in the blood of the cancerous; for if the characters of cancer could be found therein, nothing would be easier than, by examining a few drops of blood, to diagnose the disease. Microscopists found nothing peculiar in the blood; but at last they discovered a something, and, overjoyed at this, I was one of the first to applaud the achievement. But is that something sufficient? I believe not; and it is because I consider the microscopical characteristics insufficient that I have studied with the greatest care the clinical indications.

I tried, at first, to isolate benign from malignant tumours, and I commenced to separate from true cancer those tumours usually confounded with it. I have now come to the conclusion that, in true encephaloid cancer, the variety which is most prone to return, a cure *may* sometimes take place. In support of this proposition I shall cite some facts, and the Academy will judge whether they are good, or whether they deserve the severe judgment pronounced upon them by M. Robert.

In February, 1836, a woman consulted me, who had on her breast a large tumour, the size of both fists, which was ulcerated and fungous, discharging an insupportably fetid ichor, and which crumbled under the finger. There could be no more characteristic specimen of encephaloid cancer, and although all the microscopes in the world had been testifying against me, I should still have held this opinion. Well, I operated on this woman. This was nearly twenty years ago; and I saw the patient recently; she was perfectly cured, and has had no relapse.

Another lady had a tumour very similar to that which I have described, which was removed; the wound cicatrized perfectly; but, eighteen months afterwards, a tumour very like the original one manifested itself in the axilla. This second tumour was removed in 1841, and since then she has enjoyed perfect health, and has had no relapse. I may state that M. Jobert was the surgeon who operated the second time. (Here M. Jobert made an affirmative sign.) Are there not other tumours, besides cancerous, which reappear thus, not at the original point, but at a distance? We cannot tell.

I know that to these facts an objection may be raised. It may be said: "How do you know that the recurrence, which has not happened at the end of two, three, five, or ten years, may not take place in twenty, twenty-five, or thirty years?" and in this manner it may be said, if no return of the disease has occurred, that the patient has died before there was time for its manifestation.

It may be objected that, in the cases I have just reported, the microscope was not used; but since 1847, when the use of the instrument became general, every tumour that I have extirpated has been examined by means of it. To those who could say, with great justice, that I am not sufficiently qualified for microscopical researches, I would observe that the cancerous growths were examined for me by the learned young microscopists who attended my visits, by M. Follin, M. Robin, M. Broca, and M. Lebert himself. Now, since this time (1847), all the cases in which I extirpated malignant growths, and in which no recurrence took place, were previously demonstrated by the microscope to be true cancerous tumours. It may be remarked, however, that sufficient time has not yet elapsed to enable us to say with certainty that these patients are exempt from any chance of a relapse. But we know that when recurrence of the disease happens, it is generally within two years after the operation, and that after this period the chance of this becomes much lessened. Now, these patients have been free from disease for *seven* years. I think, therefore, that it is proven that cures have happened in cases where the existence of cancer had been microscopically examined. I maintain that errors of diagnosis were impossible in the cases I have cited.

M. Robert thinks that I confound different things together; as, for example, epithelial and fibro-plastic tumours. This objection brings me back to the microscope, on the use of which I wish to make a few remarks.

Laënnec divided tumours into two classes, which are merely those called, now-a-days, homeomorphous and heteromorphous growths. Among the *heteromorphous* tumours we class encephaloma, schirrus, phagedenic ulcers, etc. Among the *homeomorphous* we rank fibrous and fibroid tumours, cutaneous excrescences, condylomata, corns, fungosities, etc. Can it be said that there is the least analogy between a malignant fatal tumour and a corn on the foot? The microscope reveals the same minute structure in a venereal vegetation and in an excrescence (*verrue*). Are they, then, the same thing? M. Lebert having remarked that the tumours in question contain only epithelial matter, concludes that they are not cancer. He must have seen many cases, however, since the publication of his *Traité de Physiologie Pathologique*, to convince him that it is very difficult to reconcile clinical facts with the results of microscopical observations.

He says that the composition of these tumours explains their non-recurrence; but this is erroneous, for epithelial tumours return like others. Nay, more, I declare that they return *oftener* than other tumours. However, M. Lebert said to me: "You do not distinguish, but confound together tumours which are different; those observations only are valuable which are confirmed by the microscope, and all preceding ones are worthless." I cannot entertain this objection. However, I commenced investigating anew; I submitted cancers of the lips to M. Lebert; he found them epithelial, and consequently pronounced them to be non-malignant ulcers. Yet, in the most of these cases, the disease returned after the operation. In the majority of cases, ulcers of the lips are epithelial tumours, and they return like cancers. M. Lebert has been obliged to acknowledge this; but he says that, unlike cancer, they grow on the same place, and produce no general infection. I would answer, that cancer also sometimes returns on the same place; and I would ask if it is true that epithelial growths only return on the same site? I am inclined to dispute it; and Lebert and his disciples have attempted to get out of the difficulty by saying that, when epithelial cancer does not return at the same place, it recurs in the neighbouring lymphatics (*ganglions*); that is, in tissues directly continuous with those in which the malady primarily appeared. But does not this also happen in cancer?

Homœomorphous tumours, therefore, are exactly like cancer, for they recur both at the same place, and at a distance from it. The micrographers say that they never occur except in the neighbourhood, or in the lymphatics. I need not, in answer, adduce old cases of recurrence in the viscera, for in these the growths were not microscopically examined; but I may point to recent cases in which homœomorphous tumours recurred in different tissues from those primarily affected—as in the bones.

The treatment of the two classes of tumours is the same; we do not apply caustic, the cautery, or the knife, any less to epithelial than to cancerous growths. Wherein, then, do they differ clinically? It may be said that epithelial tumours do not cause death so speedily as cancerous, the latter often recurring in the viscera. But if both are fatal, what matters it where the recurrence happens? To recapitulate: epithelial tumours recur after extirpation, not only on the original site, but, like cancer, at a distance from it; they return alike in homologous and heterologous tissues, in the bones, and in the viscera. Call them epithelial cancers, if you will, to distinguish them from cancer, properly so called, and from scirrhus or encephaloma, but they will always be regarded as cancerous by me.

The so-called fibro-plastic tumours are said to be another variety of homœomorphous growths; but I cannot accept this distinction as a decided fact. It is said that they are simple hypertrophies of a normal tissue, or a production of a tissue analogous to normal tissues. They are evidently another thing. How can we compare, for instance, the enormous fibrous tumours sometimes found on the shoulder; and yet the microscope reveals the same texture in both. Keloid is also fibro-plastic tissue; but it does not resemble the growths of which I have spoken, and yet they are not distinguishable by the microscope. I have seen enchondroma return; I remember removing a tumour of this kind from the foot, which reappeared, after some years, on the thigh. You say, perhaps it was cancer; we need more of such cases. There have been more similar to this, and confirmatory of it. I will quote one: A man presented himself at *La Charité*, having a tumour which had grown on the site of a similar one removed by M. Malgaigne; and, some years before that, a tumour of the same kind had been excised by M. Blandin, so that this was the third relapse. The new tumour had burrowed under the cicatrix of the last operation. In spite of the small chance afforded by another operation, I yielded to the earnest solicitations of the patient, and removed the growth, cutting round it very deeply, so as to make sure of leaving none of it in the wound. All went well, the patient seemed convalescent, when I observed, before the cicatrix had formed completely, a nodule commencing below it. We allowed the wound to cicatrize, and I sent him away without saying anything, firmly resolved to make no fresh attempt. Some time after, the tumour was again developed, and the patient, knowing my determination, went to the *Hôtel-Dieu*, to be under the care of M. Roux. Amputation of the thigh was performed, and he left the hospital. He entered the *medical* wards of the *Hôtel-Dieu* some time afterwards, complaining of something in the chest. All at once a fresh growth had occurred on the leg, and simultaneously tumours of the same nature had developed themselves in the thoracic viscera. The patient died, and his body was examined by MM. Follin, Giraudet, and Verneuil, who found in those tumours nothing but purely fibro-plastic tissue.

I could cite many similar cases; but one or two must suffice. A most beautiful young woman had an enchondromatous tumour on her arm, which had increased to such an extent that her only chance of cure lay in disarticulation of the arm. The patient declined the operation, but remained in the hospital. The tumour grew as large as the head, and she died, and at the autopsy similar tumours were found in the lungs. M. Richard, a great partisan of the microscope, examined these growths, and found them to be composed of only fibro-plastic tissues. I remember another case of enchondroma of the foot, which returned after removal; I amputated the leg, and it recurred on the thigh; amputation of the thigh was performed, but the tumours became developed in the lungs.

M. Lebert is aware of these facts, and he admits the recurrence of homœomorphous tumours on the original place, and even at a distance in the neighbouring lymphatics. But that is not enough; he could not deny the existence of cancerous cachexia in an individual having cancroid who had become emaciated, and had a sallow icteric tinge. *There are several species of cancer—cancers with and without cells.*

As regards the cancer-cell, what have we to prove that it is the pathognomonic character of cancer; that it is not, for example, a transitory condition as yet but imperfectly understood? At present, there are two classes of microscopists, viz. the clinical and the scientific; the latter variety, who are principally in Germany and in England, naturally attach an absolute value to microscopical characters. The clinical micrographers are less absolute, and I have more confidence in them. Their assertions, however, must be taken with some reserve. For example, Lebert says: "There is nothing pathognomonic of cancer, except the cell." Now the cell is not pathognomonic, for we now know that it is the nucleus which is most important. But I am not more predisposed in favour of the nucleus than the cell. Very likely the nucleolus may, next year, be regarded as the all-important element. I sincerely trust that microscopists may arrive at something definite. If I have been somewhat severe upon them, it is because I think they have been too rash, and because I dreaded their going astray. They have affirmed that the cell is pathognomonic of cancer, and when experience demonstrated that their proposition was not always true, they actually tried to explain these pretended anomalies by an alteration of the cell, capable of making it momentarily disappear. If the cell is not *constantly* found in cancer, it cannot be said to be a pathognomonic mark of it.

The truth is, whatever be the explanation of it, *the cell does not always exist in true, well-developed cancer* (I speak of encephaloid); *and it is sometimes found in tumours which are evidently not cancerous.* I could quote many illustrative cases, and especially that of a woman with a non-malignant tumour in which the cells were found, who never had cancer, and who was not the least likely to have it. Many years ago, I removed a sanguineous cyst from the lip of a young girl, in the matter contained in which, M. Lebert found cancer-cells. The girl recovered perfectly; she has since been married and had several children, and is now in the best health. I removed a non-malignant tumour of the calcaneum, in which the microscopists found cancer-cells. The following seems to me very conclusive: A woman, about forty, had, on the right breast, an encephaloid lardaceous tumour, of that worst kind which returns so soon, and always fatally. As the woman was young and fresh-looking, I removed the growth. The tumour was lardaceous scirrhus, through which were scattered, here and there, portions of cerebriform tissue, not yet softened. I cut this tumour into five pieces, one of which I sent to each of five microscopists, asking their opinion of it. *None of them could find a cancer-cell in it.* The poor woman appeared at first to convalesce, and to confirm the diagnosis of the micrographers. But, after a few months, a small tumour appeared near the cicatrix, which was soon followed by general infection, and she died ten months after the operation. A few years ago, I performed amputation of the thigh, on a lady, for a tumour containing cancer-cells, and she is now in perfect health. Fifteen years ago I performed amputation for a disease of the external condyle of the tibia, containing cancer-cells, and the patient is still alive and well.

I may remark that cancers containing *no* cells have occasioned the recurrence of cancers containing cells. We must, therefore, admit one of two things—either that the recurrence was not of the same nature as the primitive malady, or that the first tumour was the same as the second, but containing no cells. I recollect a case where a tumour of the jaw recurred. There were cells in the second tumour, but none in the first. Must we, therefore, conclude that the first tumour was epithelial and the second cancerous? Assuredly not. The nature of a tumour cannot be judged of merely by the presence or absence of cancer-cells.

All these facts constrain me to declare that the microscope has not as yet aided much our diagnosis of cancer, and the question of its curability or incurability. I believe that there is in cancer some unknown and peculiar element which has hitherto escaped our observation. Else why should not the microscope be able to distinguish those fibrous tissues which recur from those which do not? or why should it not explain these recurrences? But, on the other hand, I am convinced that, in doubtful cases, the microscope may do us great service by confirming our diagnosis. The microscope is an additional eye which we possess; but we should never forget that this eye cannot enable us to see that which does not exist. It is difficult to avail ourselves of the instrument, and this is demonstrated by the frequency with which we see the most expert microscopists at variance among themselves.

This is only a part of what I would say on the subject. The continuation of the discussion may probably furnish me with an opportunity for still further giving expression to my sentiments.—*Monthly Journ. Med. Sci.* Nov. 1854.

35. *Difficulty of Determining the Structure and Nature of Cancer.*—Some time since, the Physiological Society of Edinburgh appointed a committee to report on this subject. At a meeting of the Society, on the 22d of last April, Prof. BENNETT stated that the committee had been dissolved without arriving at any report. In proposing the formation of that committee, Dr. Bennett said he had conceived it possible for four or five members of the Society, all of whom were acquainted with the present state of our knowledge concerning cancerous and other growths, to arrive at definite conclusions as to the structure, nature, and definition of such formations. He regretted to say that the committee could not come to a unanimous opinion on any one point concerning them, from which might be inferred the excessive difficulty of framing satisfactory conclusions on the subject in the present state of science. He hoped, however, that on some future occasion, when our knowledge was more advanced, some other committee might prove more successful.—*Monthly Journ. Med. Sci.* Nov. 1854.

36. *Researches upon the Structure of Erectile Tumours, and upon a New Form of Tumour confounded with them.*—Prof. ROKITANSKY has already described, in his *Treatise on Pathological Anatomy* (1846, vol. i. p. 276), under the name of cavernous tumours, erectile tumours as products of new formation. More recently, in an essay published in the *Transactions of the Academy of Sciences* at Vienna (1852), he brought forward additional arguments in support of his former opinions. The present work completes his observations upon this form of tumours, which is particularly interesting to the surgeon. The stroma of erectile tumours is formed by a network of fibres analogous to those of cellular tissue, the thickness of the septa being very variable. From the thicker septa there radiate others more delicate, which incompletely circumscribe irregular spaces communicating one with another. In these spaces there is found blood, liquid or coagulated, and concretions resembling phlebolithes. The seat of these tumours is very variable; most commonly they are met with in the liver. Then, in order of frequency, should be cited the subcutaneous cellular tissue, the dermis, the face, the trunk, the limbs, the bones of the skull, the dura mater, and the pia mater. They are always attached to veins, which penetrate in general into their texture. The erectile tumours of the liver correspond with the ramifications of the vena portæ. Surrounded by a fibro-cellular capsule, these tumours exert, by their pressure, an injurious influence on surrounding structures. The primitive element of development in these tumours consists, according to the Viennese Professor, in transparent, smooth, but sometimes slightly striated fibrils; moreover, there are found oblong nuclei and caudate, fusiform cellules. It is uncommon to meet with an epithelial in-

vestment. Organic muscular fibre has not been seen by the author. The principal demonstration of the independence of these erectile tumours from surrounding vessels, at the commencement of their formation is derived from the examination of the liver. In this organ there are met with, by the side of hollow tumours, others completely analogous, but in which the vessels are not yet developed. The blood does not enter the tumour but by the perforation of the vacuities. Thus it will be seen why Rokitansky uses the term "cavernous tumours" in place of telangiectasis.

The new form of erectile tumour, described by Rokitansky, is of glandular and lobulated appearance. Each lobule is maintained in its proper relation by a sort of cellular tissue, which sometimes contains fat lobules. This form of tumour has been observed twice in the upper lid, and once in the skin of the arm. The lobules of pinkish-red colour, contain vascular loops, which resemble the substance of the kidney. Each vascular loop is formed of two fine superposed structureless membranes. Moreover, oblong nuclei and caudate cells correspond with the loops. The loops furnish ramifications which terminate in a cul-de-sac. Rokitansky regards these vessels as of new formation, and without connection with the normal canals of circulation.—*Med. Times and Gaz.* Oct. 14, 1854, from *Zeitschrift. der K. K. Gesellch. der Aertze zu Wien. von Hebra*, 1854.

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37. *Pathological Anatomy of Ranula.*—Dr. C. O. Weber relates (*Virchow's Archiv.* Bd. vi. Heft. 2) the particulars of two cases of ranula, treated in the Surgical Clinique at Bonn, and presents some remarks to illustrate the pathology of the disease, which, notwithstanding the researches and writing of many authors, seems to be yet involved in some obscurity.

A smith, aged 33, applied, in the winter of 1851–2, at the Surgical Clinique, in consequence of a plastic membranous exudation on various parts of the mucous membrane of the mouth, following an attack of chronic diphtheritis. The use of the knife and scissors, and the application of hot iron, ultimately got rid of the affection; but, in May, 1852, there formed, close to the frænum linguæ, on the right side, a ranula, which required many operations for its complete obliteration. The patient, who was also suffering from hydrocele, left the institution in July, but returned in October, to have the radical operation performed on the tunica vaginalis. The tumour under the tongue had reappeared, and required the reapplication of the knife and cautery. In January, 1853, the ranula had formed again, having acquired the size of a hazelnut. The contents were squeezed out, and collected for examination. The greater part of the cyst was excised, the operator having seized it with hooked forceps; the small piece which remained was rubbed with caustic. This proceeding seems to have been successful, for there was no return of the disease in January, 1854, when the patient was again seen on account of the hydrocele. The extirpated piece of the cyst was examined. It was covered by the mucous membrane of the mouth, and was composed of firm areolar tissue, containing many bloodvessels. The fluid was thick, greenish, and transparent, and contained epithelium and mucous corpuscles. Acetic acid caused immediate coagulation.

Catherine Korff, aged 27, was the subject of ranula on the left side of the frænum linguæ, for which Dr. Writzer performed the following operation: He punctured the cyst with a trocar to collect the fluid pure. The anterior part of the cyst was then removed, and the remainder touched with tincture of iodine. The case terminated favourably. Dr. Boedecker examined the fluid, and pronounced that it contained neither albumen nor paralbumen in remarkable quantity, but some pyin. Its chief constituent (besides water) was mucus. The characteristic constituents of saliva, namely sulpho-cyanate of potash and phyalin were absent.

Thus, the chemical analysis of the fluid of ranula does not allow us to pronounce the disease to be the dilatation of the duct of a salivary gland; it rather shows the analogy with synovial sacs and mucous cysts. Anatomical investigation points out the following structures under the tongue capable of being developed into cysts: 1. The salivary glands and their ducts, namely, the submaxillary and the sublingual glands and Wharton's duct. But these ducts are composed of a firm areolar tissue, with elastic and flat muscular fibres; the interior is covered by cylindrical epithelium. The cyst of a ranula is formed only of areolar tissue, and contains a little tessellated, but no cylindrical epithelium whatever. 2. The mucous glands of the membrane of the mouth. Dupuytren believed that obstruction to the duct of one of these glands might produce ranula; and this view is somewhat supported by the examination of their minute structure; but the author rejects it upon anatomical grounds. 3. The synovial sac described first by Fleischmann (De novis sub lingua bursis, Nuremberg, 1841), the presence of which has been confirmed by Froriep. It lies close to the frænum linguæ, on the outer side of the genioglossus under the mucous membrane of the tongue, sometimes only on one side, sometimes on both, but never between the two muscles. The author, after pointing out the necessity of taking all these structures into consideration in the examination of cystic tumours in the floor of the mouth, expresses his belief that the cysts described by Fleischmann form the ranula commonly seen in surgical practice.—*Med. Times and Gaz.* Sept. 30, 1854.

38. *Treatment of Aneurism by Compression.*—Joliffe Tufnell, Esq., has published (*Med. Times and Gaz.* Oct. 14, 1854) some interesting remarks on this subject in reply to the observations made by Mr. Skey in two lectures. (See *Med. Times and Gaz.* Sept. 23 and 30, 1854.) Mr. Tufnell restricts himself to noticing two questions propounded by Prof. Skey:—

The *first* point is the difference in the success which has attended the adoption of this line of treatment in Dublin and in the metropolis, and he expresses a desire "to ascertain how far this success is to be attributed to the fortunate opportunities of repetition of the operation enjoyed by the Dublin surgeons; whether, in fact, their success is not attributable to the experience obtained by repetition, or, in other words, whether failure was not a more common attendant on their early than their later cases?" To this Mr. T. answers, no; the proportion of successful cases will be found to be as great during the first years of the introduction of compression as in that just gone by.

"Now, to enable the profession to judge of the accuracy of the statement," says Mr. T., "I here make, I beg to offer to their notice the statistics of this disease as treated in Dublin by compression, from its revival in 1842, down to the present time:—"

[The table gives the particulars of 47 cases of aneurism treated by compression, in Dublin, from Oct. 1842, to Sept. 1854; 6 were treated at Richmond Hospital; 9 at Steeven's; 6 at St. Vincent's; 3 at Jervis-Street; 7 at the patients' residences; 3 at Meath Hospital; 5 at the Royal Military Infirmary; 1 at Adelaide Hospital; 3 at the City of Dublin Hospital; 2 at Mercer's Hospital; 1 at the Artillery Hospital; and in 1 the place of treatment is not stated. Out of these 47 cases, there were 36 cures by compression treatment. In two cases the patients were females—1, 22 years of age, duration of compression 20 days; the other, 40, duration of compression 9 days; the latter right brachial, and the former right femoral—both cured. Of the 45 males, 1 died of disease of the heart, duration of compression 20 days; 1 of erysipelas; 1 of disease of heart and lungs; and 1 is reported as unsuccessful. Of the males, 15 were of right popliteal aneurism; 15 of left; and in 4 not stated; 4 of right femoral; and 3 of left; 1 of right brachial; and 1 brachial, side not specified; 1 of left radial; and 1 of right ulnar traumatic. In 7 cases they were the same patients as had been previously treated.

Most of the cases are reported in the *Dublin Medical Press*, the *Dublin Journal*, the British Association Reports, the *Dublin Quarterly Journal*, the *Dublin Hospital Gazette*, and 4 in *Tufnell on Compression*, pp. 125, 128.

Under Dr. Hutton . .	5	Under Mr. O'Ferral . .	2
" Mr. Cusack . .	7	" Dr. Macdonnell .	1
" Dr. Bellingham .	5	" Dr. Humfrey . .	2
" Dr. Harrison . .	1	" Dr. O'Brien . .	1
" Dr. Kirby . .	1	" Mr. Smyly . .	1
" Mr. Porter . .	2	" Dr. Orr . . .	1

Under Dr. Fox	1	Under Mr. Tufnel	3
" Sir P. Crampton	1	" Dr. Quigley	1
" Dr. Banon	1	" Mr. Colles	3
" Dr. Clayton	2	" Mr. Fleming	1
" Dr. Read	1	" Mr. Wilmot	2
" Dr. Hargrave	1	" Dr. Jameson	1

The ages of the male patients were as follows:—
11—20—24—^{3}25—26—^{3}27—^{3}28—^{4}29—^{5}30—31—^{6}32—^{2}33—^{3}34—36—37—38—40—43—44—45—46—48—55. 1 not stated.

The shortest duration of compression was one of 7 hours; the longest 93 days; the several periods were as follows:—
^{2}2 days—4—5—6—^{2}7—8—9—11—12—13—^{2}20—^{2}21—24—28—30—31—33—37—42—^{2}43—53—70—72—93. And 7 hours—10—11—16—23—30—33—39. In 8 cases the duration is not stated.

In one case of popliteal aneurism, ligature was applied, and the patient recovered; in another, of right popliteal, cured by compression, the left femoral had been previously tied for left popliteal aneurism. Interrupted pressure was applied in one case, cured. In a case of right brachial, in which ligature was applied, there was high bifurcation, and two vessels were secured. In a case of left femoral, and in two of right popliteal, amputation was performed, and recovery took place; in another of left popliteal (male—25), ligature was applied successfully; and Dr. Fleming remarks upon the case: "It was decided that the treatment by compression should be tried, though no sanguine expectation was entertained as to its success, as well from the nature of the aneurism, as from the character of the patient." In a case of left popliteal, cure was effected by manual compression made by the patient's own thumb upon the artery at the groin. In another case, in which ligature was applied unsuccessfully, the knee-joint was involved, the aneurism springing from the anterior aspect of the artery. In a case of common right femoral, cured by compression, the sac sprang from the femoral so immediately below Poupart's ligament, that one point only existed where pressure was complete, so as not to admit a drop of blood into the sac. The man was well lowered by croton oil previously.]

In submitting this statement, I would beg emphatically to call attention to one fact—viz. that it contains *every single case* that has occurred in Dublin in which compression has been employed. It has not been compiled (as the statistics of ligature have) from certain published cases only; it includes every case of aneurism where a compressing instrument has been placed upon the limb; and, to show how general has been this treatment, I believe there have been three instances only during this time in which it has not been tried, two being ligatured at once, and amputation resorted to in the third. The Profession, therefore, have here brought before them the practical working of compression in the treatment of aneurism in Dublin, and can judge of the result. It will be seen that the statement embraces a large number of names, some of whom have had only a single opportunity of treating the disease, and yet success in their hands has been equally complete with those having a larger number of cases. The "experience of repetition" is not, therefore, necessary for success. It is assuredly a great advantage to have had practical experience, especially in the management of details; but, the principles of compression once established and understood, *judgment in selection, and watchful care in the management of the cases*, are alone required.

It is a mode of treatment, however, far different in its application to the ligature. With the ligature, so soon as once applied, the surgeon has nothing further to do than wait patiently for nature to perform her part, and to guard against the dangers of hemorrhage; but, in compression, as long as the sac is pervious to the influx of blood, so long must unremitting attention be given to the case; for it is ever to be borne in mind (and the fact cannot be too strictly enforced), that, carelessly or injudiciously employed, pressure is fraught with danger to the patient. While the compressing pad is on the artery, progress toward recovery may be presumed to be going on; but the instant it shifts from off the vessel, that moment is the treatment not only rendered nugatory, but prejudicial in the extreme, tending to engorgement of the limb, and diffusion of the contents of the aneurismal sac. I would strongly urge this fact upon the notice of the profession, because I have frequently heard the remark made by men who had not considered the subject, that compression ought to have a trial in all cases, because it can do no harm. Ill applied, I say, it not only can do no harm, but as assuredly will do so, as, well applied, it may be with confidence looked to for a favourable result.

The second point on which I have to remark is with reference to elastic pressure, and its desirability of adoption in practice. On this head, Mr. Skey seems to have mistaken my views; for he says, by "approving of the weight I have rather shelved the question of elasticity." I do most certainly uphold the use of the weight for controlling circulation through the femoral artery at the groin, and elsewhere that it can be brought to bear; but this does not in any way influence my sentiments as regards elastic, and its superiority over unyielding pressure. My opinion I have already strongly and decidedly given, and I cannot, I believe, more clearly express my ideas upon the subject than by quoting the words I have used in my published lectures:[1] "An elastic medium has been introduced, in place of that previously employed, viz. the dead, unyielding screw. This I consider the main, the great improvement. The form of mechanical arrangement for bringing this force into operation may be varied in a thousand ways; but the elastic medium was the desideratum required, and that has now been gained by the appliance of Dr. Carte."

No language can, I think, be more decided than this, as expressing my opinions on the subject. Mr. Skey, however, does not equally value it, and seems to doubt the necessity for its introduction into practice. He says: "If you require a certain amount of pressure for any given purpose, it appears to me that it cannot signify in what form the pressure is made. If elastic material be employed, it is necessary to screw down the pad to a pressure equivalent to the required weight. Elasticity of pressure implies a varying action of the instrument, the pressure on the artery being greater or less as the elasticity is brought into play. But, by what means can the elasticity be made serviceable, or at least appreciable? It is obvious from its construction that this elasticity cannot be obtained (in the instrument recommended by Mr. Tufnell—Dr. Carte's), from above, or inwards, but only by pressure from the centre to the circumference, that is, pressure of the integuments against the pad, and not of the pad against the integuments. How is this to be effected? for if we allow that the pressure is already sufficient, any amount beyond that is superfluous and injurious. But the limb is at rest the most absolute, and one does not see in what manner pressure from the limb against the pad can be effected. On full reflection, I confess my inability to appreciate the merits of the so-called elastic pressure; for it does not appear to my mind to involve the principle."

Now the explanation of the means by which the elasticity is brought into play, and pressure made from the centre to the circumference, is simple. It is this: The pressure of the pad, after a while, causes a degree of uneasiness, which, long continued, amounts to pain. To relieve this, the muscles of the part contract, and in doing so raise the integuments against the pad; this alteration of position of the fibre, slight as it may seem, being sufficient to afford relief. In this relief to the compressed muscular fibre does the whole secret lie. None such is afforded by the screw; when once applied, the whole limb lies fixed, that flexion or extension is forbidden of which the elastic pressure admits; and, consequently, a degree of pain and constitutional disturbance often follows, which, in an irritable patient, may mar the whole results.

I am not speaking from theory, I appeal to experience for confirmation of my views, and I find it ably replying. I quote, as proof, from the writing of Dr. Hutton.[2] He says: "We could effect little or nothing with the screw clamp, from the patient's impatience of the pressure; but he was able to bear the elastic instrument of Dr. Carte for six hours, although the compression was equally complete, and pulsation prevented. In this case, after the application of the instrument, the pulsation ceased, and never after returned."

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[1] The Treatment of Aneurism by Compression. Dublin, 1851. P. 55.

[2] Dublin Medical Press, May 16, 1849.

In another case, Dr. Hutton remarks: "The patient sustained the compression during seven and a half successive hours, never allowing pulsation to return to the tumour. At the end of this time he removed the instrument; pulsation had wholly ceased; the aneurismal tumour became solid, and absorption soon commenced. I feel confident that this patient could not have borne the application of the screw clamp for so long a time." These are tests of the efficiency of elastic pressure which, I think, cannot be gainsaid. These are the results of its working in the treatment of disease in Dublin. An easy mode, however, of establishing its superiority exists, which any member of the profession who doubts can test upon himself. Let him place a screw clamp upon the femoral artery at Scarpa's space, or on any other portion of the thigh, so as to stop pulsation in the vessel below. Let him then try to walk across the room; he will find himself, as it were, rivetted to the spot, totally unable to move. Let him next substitute for the clamp, a circular compressor of Dr. Carte, and equally control circulation through the limb, and then try his powers of progression. Instead of inability to move, he will discover that he can proceed freely, and walk with ease, the piston of the pad rising and falling to each step as the muscles contract and extend, keeping still the artery compressed, and allowing no blood to pass through the point of arrest. "That eventually compression will become the general mode of treating aneurism of the extremities I have not a doubt;" and the concluding sentence of Mr. Skey's able lecture confirms the opinion I thus expressed in 1851. Mr. Skey says: "Notwithstanding temporary obstructions, we may look forward with the utmost certainty to the treatment of aneurism by compression as the future law of our profession." I have replied to these two points raised by Mr. Skey, because I consider them all-important—the statistics, as indicating a success in Dublin that may stimulate others to adopt this mode of treatment elsewhere; and the form of pressure most desirable in the treatment, as so mainly influencing the well-being and successful termination of each case.

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39. *Tumoral Aneurism cured by Compression.*—Mr. M. H. Collis records (*Dublin Quarterly Journal*, Nov. 1854) an interesting case of this, in which compression was applied close to the sac, and appears to have acted on a different principle from what has hitherto been claimed for it.

"Christopher Toole, aged 32, by trade a nailer, a tall, athletic man, of dark complexion, presented himself among the extern patients at the Meath Hospital, on Monday, July 24, 1854, with a pulsating tumour on the upper part of the right thigh. This tumour was 4¼ inches long, and 5½ wide; the circumference of the limb over the centre of the tumour was 21¼ inches, that of the corresponding part of the sound limb being 18½. It occupied the upper part of Scarpa's space; its upper margin was in contact with the depression which marks the position of Poupart's ligament, and it extended downwards along the course of the common and superficial femoral vessel. The pulsation was very strong and diastolic, and the action of the artery above it was so forcible as to lead some persons to conclude that it was considerably dilated; by pressing upon the abdomen firmly we could trace the pulsation in the external iliac for a distance of nearly four inches. The superficial veins of the thigh were remarkably large, knotted, and tortuous; the glands in the groin were somewhat swollen; and upon the outer aspect of the tumour two glands could be felt elongated and flattened, and partially sunk into it; the entire limb was enlarged, slightly œdematous, but of natural temperature and sensibility. There appeared to be some solidification of the tumour on its outer margin; subsequent observation showed this to be at least partially deceptive, and resulting from the varicose condition of the veins on the internal side. Pressure upon the artery in the groin readily checked all pulsation, but without any sensible effect upon the volume of the tumour. No bruit de soufflet was audible in any position, nor was there any fremissement; the whole arterial system was excited, and the man complained greatly of the throbbing; even the momentary pressure of the thumb upon the artery gave him great relief. He stated that his general health was good, and that for the last two years he had been temperate; his occupation rendered stooping necessary, and also constant rotation of the body at the hips, the feet being kept unmoved. The floor of his workshop was uneven, and his right foot had for some weeks rested habitually on a lower level than the left. The tumour made its appearance, as far as he was aware, spontaneously, about six or seven weeks before his application at hospital; it was then about the size of a nut, and had ever since grown steadily larger.

Tuesday, 25th.—After administering purgatives, &c., compression was attempted; various instruments were tried, by none of which could the artery be commanded, even partially, for more than a minute or two. The only point available for pressure was just above the sac, at the spot where the epigastric and circumflex vessels are given off. As the brim of the pelvis is rounded just at this place, all the instruments were displaced by the ordinary respiratory movements, and slipped off the artery; a relay of pupils kept up pressure sometimes by the finger and sometimes with the clamp or leaden weight; but it was partial and irregular, in spite of the greatest vigilance and anxiety on the part of all, including the patient himself. Low diet was strictly enforced, and purgatives of croton oil were occasionally exhibited. Quiet was procured by opiates. Chloroform was administered once, but produced excessive headache. An attempt was made to draw some blood upon the second day, but it was thick and carbonaceous, and would not flow. On Saturday, after upwards of four days' ineffectual attempts at compression, the skin in the groin began to chafe and suppurate round the root of every hair, and it became evident that pressure could not be borne much longer; the aneurism had increased half an inch outwards and downwards, and the pulsation was as violent as before, compared with the general force of the circulation; no attempt at consolidation had taken place, and all parties were inclined to be disheartened.

On Sunday, the 30th, I determined to make another effort to command the vessel completely. Having procured a double clamp tourniquet, which closed by a screw at the hinge, I applied it, placing a small roll of bandage an inch thick under the pad. I screwed it home, and found that it controlled the pulsation for a moment, and, like all the rest, slipped off towards the abdomen. By screwing it a little tighter, and drawing it downwards by a tape from the knee, I was enabled at last to get the artery completely controlled. Before finally adjusting it in this position, I drew up the skin, so as to get an unchafed part under the pad. The tourniquet was not disturbed for thirty hours, and at the end of this time, upon slackening it, all pulsation was found to have ceased in the tumour and in the artery as high as it had been traced before, namely, to the spot where the common iliac bifurcates.

About twelve hours after the tourniquet was applied, the man felt a stinging pain on the inside of the knee and leg, and a kind of tingling sensation along the outside of the hip and thigh. While under pressure, he took a couple of sedative draughts, containing half a drachm of chloroform.

The spot of skin where pressure had been first attempted was beginning to slough in points, but in a few days it became healthy. The tumour became a little smaller in the first few days, although the subsidence of venous congestion and œdema gave it a more prominent appearance. He never had the smallest pain nor single bad symptom after the removal of the clamp, and upon the 12th of August he got up and walked about the ward.

On the 16th he left the hospital, and returned almost immediately to his work, having been in hospital twenty-three days; nor would it have been necessary to keep him so long but for the slough of the integument caused by the first abortive attempt at compression. I saw him on the 6th of October, and found the tumour reduced very considerably in volume; the difference between the circumference of the two limbs was less than one inch.

In the mere details of this case, there is little worthy of remark beyond the rapidity and painlessness of the cure, and the fact that it is the first instance on record of the cure of aneurism of the common femoral artery by compression. There are, however, some points of general interest to which I wish to draw attention. These are:—

First. The position of the point of pressure as regards the sac and as regards the collateral branches. The artery was compressed close to the sac; the point of pressure covered the usual origin of the superficial circumflex ilii and epigastric vessels, the latter of which could be felt pulsating before treatment was commenced; hence the nearest collaterals were practically the internal iliac on the one side, and the profunda on the other, and pressure was thus made between the sac and one of the next collateral branches.

Secondly. The fact that under these circumstances the aneurism increased in size, as long as pressure was incomplete, and upon the artery being completely controlled coagulation was rapidly obtained.

Thirdly. The simultaneous blocking up of the artery above the point of pressure, as high as the next collateral branch.

Fourthly. The non-occurrence of secondary aneurism, or of suppuration of the sac.

Fifthly. The bearing of this case upon the question of pressure at the distal side.

The immediate proximity of the point of pressure to the sac appeared to be a dangerous though unavoidable complication; it was, however, not only unavoidable, but essential to the cure. As but one point existed where pressure could be made, it was necessary that the cure should be rapid, or the skin would have failed us. Ignorant of the principle which appears to guide the cure in pressure close to the sac, we at first were satisfied with endeavouring to cut off the impulse of the heart without completely shutting off the current. As long as we did so the sac continued to increase in size, and to pulsate with as great a vehemence as ever. No sooner was the current completely intercepted from the sac by pressure between it and the nearest collateral, than coagulation of its contents took place. No current through the sac was now practicable, and the consolidation was, as we must suppose, not only rapid but complete. In compression at a distance from the sac, as in ligature similarly situated, blood is deposited layer by layer, until not only the sac but the artery at the injured part is filled up. No such process could have occurred in this case, for no branch existed between the sac and the point compressed, and therefore no current could have made its way through it. The effect of thus converting it into a *cul de sac* was the coagulation *en masse* of its contents. The same result took place in the *cul de sac* above the tourniquet, the artery becoming obliterated in a similar rapid manner. To this latter circumstance is probably to be attributed the non-occurrence of relapse, or of suppuration in the sac.

This case seems to suggest the possibility of curing aneurism by distal pressure, under favourable circumstances. If we can effect the consolidation of an aneurism by converting it into a *cul de sac*, there is reason to believe that with due regard to preparatory treatment this result might in some cases be obtained by choosing a spot between the sac and the distal collateral branch as the seat of pressure. We see in this case that not only the sac was filled up, but the artery also, which was exposed to the pulsatile wave of blood. When pressure is made on the distal side of an aneurism, we observe an increase of impulse for a few moments. If the compression is *complete, and firmly kept up*, this momentary irritation subsides, and the aneurism pulsates with perhaps less force than before. This principle of converting the aneurism into a *cul de sac* appears to act occasionally in compression at a distance. Dr. Carte, who has had great opportunities of observing the various phenomena which arise during the cure of aneurism, has stated it to me as his conviction that many, if not all, of the rapid cures are effected by a loose clot blocking up the distal portion of the artery. Certainly we can scarcely conceive it possible that a cure which occupies seven, ten, or even thirty or forty hours, could be effected by the slow process of a deposit, layer by layer, until the sac is filled. We see, also, that a similar result has been obtained purposely by manipulation of the sac, a process which is too dangerous to be other than exceptional in application. From all these circumstances, therefore, I think we may infer the probability of distal pressure being occasionally useful; it is at least worth a trial, where the position of the sac prevents our using the ordinary method; and if we can get a point for compression between the sac and the nearest collateral branch, I think the result of the cases which I have now put upon record will give us encouragement, although indirect, to hope for success."

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40. *Popliteal Aneurism, cured by Compression in Twelve Days.*—The subject of this case was a coachman, 35 years of age, of thin person, middle stature, rather pale, tranquil, and somewhat feeble circulation, and good general health; admitted under Mr. T. P. Teale into the Leeds Infirmary Sept. 10, 1854. The aneurism resulted from an injury four months previously. Two months ago, a small pulsating tumour was felt in the ham, which has been steadily increasing. It is now the size of a hen's egg. The circumference of the affected knee is 15 inches, that of the sound knee 13¾ inches.

Sept. 12.—Mr. Teale ordered compression to be made in the course of the femoral artery by means of Signorini's tourniquet; the pressure to be discontinued at nights, and to be occasionally intermitted for short periods during the day, and to be practised to such a degree as greatly to mitigate the force of the circulation, but not absolutely to arrest pulsation in the aneurism; the seat of pressure to be frequently varied in the course of the artery. The patient to have meat daily, without beer, and to be kept in bed.

15*th*. Pulsation much diminished, and the tumour smaller. He feels much easier.

19*th*. Tumour smaller and firmer; circumference of knee, 14½ inches.

24*th*. Tumour much smaller, feeling perfectly solid. A very slight pulsation may be felt in the course of the tumour, but it is doubtful whether this pulsation is in the aneurism, or in a small artery passing over its surface. For greater security in this state of doubt, the pressure is directed to be continued a few days longer.

26*th*. It is now evident that the slight pulsation felt on the 24th was due to an arterial branch in process of enlargement, and not to the aneurism, which may, therefore, be considered as cured on the 24th, or twelve days after the commencement of the treatment. The pressure is now to be discontinued.

The following table, accurately recorded by the patient, shows the duration of the pressure each day:—

Sept. 12 . . .	9	hours.	Sept. 20 . . .	13	hours.
" 13 . . .	12	"	" 21 . . .	13	"
" 14 . . .	13	"	" 22 . . .	13	"
" 15 . . .	12	"	" 23 . . .	13	"
" 16 . . .	12½	"	" 24 . . .	13	"
" 17 . . .	12½	"	" 25 . . .	13	"
" 18 . . .	13	"	" 26 . . .	6½	"
" 19 . . .	13	"			

Med. Times and Gaz. Oct. 28, 1854.

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41. *Operations in Cases of Hernia when the Sac and its Contents had been reduced en masse.*—Mr. Paget has recorded (*Med. Times and Gaz.*) two cases of inguinal hernia, in which the sac and intestine having been reduced *en masse* by the taxis, the symptoms of strangulation continuing, he laid open the inguinal canal and succeeded in reaching the hernial tumour, and relieving the constriction at the mouth of the sac; in both cases with successful results.

The best collective evidences for the reduction *en masse* appears to be—1st. The signs of strangulation continuing, or first ensuing, after the apparent reduction of a hernia; 2d. The loss of a hernial sac, more of which, according to the history of the case, ought to be felt in or beyond the inguinal canal; 3d. A feeling of fulness at the internal ring, and of an ill-defined, firm swelling in the abdomen behind or below the ring; 4th. An amount of pain at and about the internal ring, which is quite disproportionate to that in any other part of the abdomen, as well as to the other signs of strangulated hernia; 5th. The unusual drawing up of the testicle. Mr. Paget then observes:—

In these particulars, the cases I have related corroborate those already published, and confirm the rule that, when these signs are found coincident, we should act in the belief that the reduction *en masse* has taken place. But the cases presented two rare and deceptive features.

1. In each of them, the pushing back of the sac was so far incomplete, that the lower portion of it, containing fluid, remained in, and in one case projected a little beyond the inguinal canal. Feeling this portion of the sac, one might have assumed that no more had ever projected beyond the abdomen. But the cases show that we must count, among the indications of this accident, not only the complete disappearance of a hernial sac, but the marked diminution of one; that it may excite a suspicion, if we can feel, in and beyond the inguinal canal, only a much smaller hernial sac than, according to the history of the case, there should be. If, in either of these two cases, the hernia had been properly reduced, its empty sac should have been felt, not only in the inguinal canal, but along the descending portion of the spermatic cord.

2. In the second case, the sac had been pushed from its connections five or six weeks before the severe strangulation ensued, and, once in the course of that time, intestine had probably been strangulated in, and returned from, the displaced sac. Such an event has not, I believe, been previously recorded; but a single occurrence of it diminishes the confidence with which we might expect that, in all these cases, there would be the history of a very recent reduction of a hernia.

The incompleteness of the displacement of the sac, to which I just now referred, suggests a rule in operating. If, in the suspicion that a hernia has been reduced *en masse*, the inguinal canal be laid open, and no sac at all be found in it, the probability of an erroneous reduction is increased; but if, instead of no sac, one be found containing fluid, we might be apt to conclude that this is the whole sac, and that the intestine has been duly reduced from it. But the cases show that the part of the sac containing and strangulating intestine may be pushed back from the internal ring, and that another part, containing fluid, may remain in the inguinal canal: they thus establish, as a rule, that, in all such suspicious cases, we must not desist in the operation without being perfectly satisfied that all is right in the abdominal cavity, so far as it can be reached from the inguinal canal. In both the cases here related, after the exposure of part of the sac in the canal, the detection of its displaced but still constricting mouth was impossible, except by such an examination as would have been unjustifiable in an ordinary case. The sac's mouth was only just within reach of the forefinger, and the sensation received by the outstretched finger touching the intestine in the sac was at first deceptively like to that from intestine in the abdominal cavity.

I would add a remark on the after-treatment of strangulated hernia as illustrated by these cases.

The abstraction of blood by nearly a hundred leeches, in the three days following the operation, in the first case, was intended against what appeared to be an active local inflammation of the peritoneum and adjacent tissues, in consequence of the violence used in the displacement of the sac and in the operation. The giving of wine on the third day after the operation in the second case was dictated by the fear that a peritonitis would presently ensue, against which treatment might be of little avail.

The effects of both these opposite treatments appeared to be good, and they indicated, as many other facts do, that there are at least two forms in which peritonitis ensues after operations for hernia. In one class of cases it appears essentially the local consequence of injury; in the other, it is also, and rather, the local indication of a general blood-disease. In the one, it is an active local disease, amenable to local treatment; in the other, it is only one part, one feature, of a constitutional disease of low type, which local treatment can scarcely affect for good.

We may probably compare the cases of the two classes with those of phlebitis after amputation; of which, some are simple local inflammations of injured veins; others are inflammations of veins, associated with diseases of many other parts, and declaratory of diseased blood. The parallel would hold in many respects, and, among them, in this, that the peritonitis or phlebitis of local origin and nature ensues very early after the operation, or is continuous (in the case of peritonitis) with that which preceded the operation; while the disease of constitutional origin, whether peritonitis or phlebitis, comes on later, and with an interval of two, three, or many more days, in which no signs of it may be detected. There are similar differences and correspondences in the earlier and the later erysipelas after operations; the former is mainly a local disease, the latter is the localized indication of a general disease; and, as in the corresponding cases of peritonitis and phlebitis, the one is a comparatively trivial, the other a very grave disorder.

Now, the respective indications of the earlier and the later occurrence of phlebitis and of erysipelas after operations, are, I believe, recognized by many; and the time that has elapsed between the operation and the access of the disease is often reckoned among the facts determining the plan of treatment. A similar rule should be held concerning operations for hernia. In general, if peritonitis ensue within twenty-four hours after the operation; or, if it existed before the operation, and continue or is increased after it, it may be assumed to be chiefly a local disease, a simple inflammation of injured parts, which may be treated by local or other bleeding, by abstinence from food and stimulus, and perfect rest; or which, if it be slight, may be left to spontaneous recovery. If, on the other hand, the peritonitis first manifests itself more than twenty-four hours after the operation, it is probably an indication of a general blood disease of some low type, for which opium and well-regulated food, or even stimulants, afford the best hope of remedy.

I need hardly say, that this difference in the time of first appearance must not be taken as alone sufficient for the diagnosis of the two forms of peritonitis, or for guidance to practice. I believe only that it may be reckoned among the most useful indications. Neither may twenty-four hours after the operation be fixed as an exact time by which, according to their advent within or beyond it, we might class all the cases of peritonitis; it is, I believe, a fair general estimate, but nothing more. Nor must the one form of the disease be regarded as exclusively a local, or the other exclusively a constitutional malady; each is only chiefly the one or the other; and, in different cases, we may find the constitutional and the local elements of disease prevailing in various proportions.—*Monthly Journ. Med. Sci.* Aug. 1854.

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42. *Radical Cure of Hernia by Iodine Injections.*—M. Jobert has presented to the Academy an account of three cases of inguinal hernia which were radically cured by injection of the tincture of iodine into the hernial sac.

The first case was that of a young man, aged 18, who was affected with a complete inguinal hernia of the left side. It did not, however, descend to the bottom of the scrotum, and below was separated from the testicle by an elongated constriction. The bowel was readily reducible by the taxis, but immediately the patient coughed or stood on his feet it was again extruded. The patient stated that his disorder in no way interfered with his ordinary occupations, but he was extremely desirous to be completely cured of it, as he was judged unfit for military service in consequence of its existence.

In compliance with the patient's wishes, M. Jobert determined to accomplish, if possible, a radical cure, by injecting the tincture of iodine. On the 12th of May, therefore, having made an incision over the inguinal canal, he introduced a fine trocar, and injected through it five drachms of pure tincture of iodine. The patient suffered some little pain in consequence of the proceeding, and, upon its completion, the wound was united by the twisted suture, and dressed with simple ointment. In the evening, there was slight swelling in the inguinal region, but no constitutional disturbance.

On the 14th, the swelling and redness were considerable, and, on removing the sutures, the wound was found imperfectly united. After some days, however, the redness and swelling disappeared, and cicatrization was accomplished. On the 5th June, the patient left his bed, walked about, and coughed without causing the least unnatural impulse of the abdominal viscera, or the slightest reappearance of an external tumour. The left testicle (that on the affected side) remained of the same size as the right. He was directed to wear a suspensory bandage for some time, as a precautionary measure.

In the second case, the patient, aged 33, was admitted into Hôtel Dieu, on the 18th of November, 1853, with a hydrocele and a congenital inguinal hernia of the same side, and the two swellings being separated from each other by a kind of hour-glass contraction of the tunica vaginalis, the upper part of the sac being occupied with intestine, and the lower with serous fluid and the testicle. M. Jobert resolved, if possible, to obliterate the tunica vaginalis, and so to cure, at the same time, both the hernia and the hydrocele. With this view, having first interrupted the communication between the tunica vaginalis and the peritoneum, by means of pressure applied to the inguinal canal, he passed a trocar into the lower portion of the constricted sac, and, after having let out the contents of the hydrocele, injected a small quantity of the pure tincture of iodine. On the following day, the scrotum was red and slightly tender, and the affected tunica vaginalis distended with effusion of serum. The patient, however, manifested no constitutional disturbance, and complained of no pain. Day by day the swelling and redness diminished, and, within seventeen days after the operation, the scrotum had regained its ordinary dimensions; but, on the affected side, both it and the spermatic cord were firmer than natural. A cylindrical cord existed through the whole of the inguinal canal, and extended along the course of the spermatic vessels to the testicle. A complete cure was obtained, and the patient left the hospital, able to walk without fatigue, and exhibiting no reappearance of the hernia, in whatever position he occupied. No serious local inflammation, nor any constitutional disorder, resulted from the injection.

The third case was that of a congenital hernia of the left side, quite reducible, and equal in size to a large pear. The patient was twenty-seven years of age, and had been ruptured eight years. He had worn various bandages, in order to counteract his infirmity, only one of which had at all answered the purpose. As the patient ardently desired to be cured of his disorder, M. Jobert, in the presence of his colleagues, punctured the hernial sac, and injected into it a small quantity of the pure tincture of iodine. The patient was then placed on his back, with his legs flexed slightly on the thighs, in which position they were maintained by a bolster. Almost immediately after the injection, the hernial sac began to swell, and in fifteen hours the sac had become as large as if filled with intestine, and communicated a fluctuating half-solid character to the finger. The parts remained in this condition for eight days, after which the swelling rapidly diminished. Twenty-eight days after the operation, the track of the spermatic cord was occupied by a cylindrical substance so compact and hard that the patient was able to rise from his bed, to walk about, and to sit down, without the smallest appearance of an intestinal protrusion exhibiting itself.

The cure remained complete four months after the operation, the testicle had not atrophied, and the inguinal canal was occupied by a firm, solid cord, into which the vaginal process has become converted by its obliteration.

M. Jobert does not allow the tincture to remain permanently in the sac, but withdraws it by means of the syringe. In all cases of congenital hernia, or whenever the sac is distended with fluid, or is thickened and condensed, and has become adherent to the surrounding structures, M. Jobert penetrates it at once with a trocar, without first dividing the skin with a scalpel; but, whenever the sac is thin, movable, and easily displaced by pressure, he prefers to divide the skin with a scalpel, and expose the sac before puncturing it.

The superiority which the method by injection offers to all other proceedings which have been devised for the radical cure of hernia, depends, says M. Jobert, on its harmlessness and simplicity; and M. Velpeau deserves the credit of having, in his *Annales de Chirurgie*, published ten years ago, first noticed the advantages of iodine injections for effecting the radical cure of hernia.—*Gazette des Hôpitaux.*

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43. *Efficiency of Ice combined with Compression in the Reduction of Hernia.*—In sixteen cases of strangulated hernia, in which all the ordinary means of

reduction had been unsuccessfully employed, M. Baudens has effected the return of the bowel by the application of ice associated with permanent local pressure. The degree of refrigeration is to be regulated in proportion to the amount of inflammation in the hernial tumour, and the sensations of the patient. M. Baudens commences by the application of a simple compress, which is soaked from time to time in water, whose coldness is gradually increased. Fragments of ice are afterwards placed on the compress, and the cold thus produced may be augmented, if necessary, to a very considerable intensity by the addition of increasing quantities of common salt. Sometimes the refrigeration alone is sufficient to effect the reduction, as M. Baudens has witnessed on three occasions; but when this does not take place, an elastic bandage is applied over the ice, by means of which a continual steady pressure is exercised on the tumour. Whenever the rupture is so painful that no pressure can be tolerated, ice alone must be employed at the outset, and the elastic bandage may be added as soon as the sensibility of the swelling is sufficiently diminished. In order to facilitate the action of these measures, the pelvis should be kept in an elevated position.

M. Baudens ascribes the efficacy of this treatment to the powerful influence of cold in diminishing the size of the hernial tumour, by overcoming the capillary congestion, and extinguishing the inflammation in the strangulated bowel. Hitherto, says he, the treatment by cold has been condemned, because its therapeutical effects have been neglected or misappreciated. He regards the idea that the application of cold to a strangulated rupture is likely to produce mortification in the bowel as a groundless apprehension, believing that so large an amount of heat is developed in a part during the continuance of inflammation, as to enable it to resist with facility long and powerful refrigeration, without at all endangering its vitality. He admits, however, that it is quite possible to continue the application of cold too long, but maintains that the feelings of the patient furnish the best index as to the advantageous or prejudicial operation of the remedy. As long as the patient feels that the action of the refrigerant is grateful, and productive of comfort, its application should be continued; but, as soon as he begins to experience a disagreeable sensation of coldness and moisture, it should be immediately removed.—*Med. Times and Gaz.* Sept. 30, 1854, from *Gazette des Hôpitaux*, June 3, 1854.

44. *Arrest of Hemorrhage from a Burst Varix.*—At the bedside of a patient now under his care, in the London Hospital, on account of a ruptured varix in the leg, Mr. **Adams** took occasion, a few days ago, to point the attention of his class to the practice which should always be resorted to in such accidents. The bleeding was, he remarked, from the *proximal* end of the vein. There often existed either none, or very imperfect valves between the injured spot and the heart. In such a state, the only propelling force would be the mechanical pressure of the column of blood above the laceration, the effect of which might be in a moment reversed, by the simple measure of *elevating the leg.* On referring to several surgical works, all of which mention cases of death from hemorrhage under these circumstances, and correctly explain its cause, we do not find any allusion to the method of arresting it recommended by Mr. Adams, and have, therefore, esteemed it worthy of mention here. None who have seen cases of burst varix will ever forget the fearful way in which they sometimes bleed, and most will doubtless have encountered some difficulty in securing arrest by pressure, as is usually attempted.—*Med. Times and Gaz.* Sept. 9, 1854.

45. *Cancrum Oris treated by the Application of Nitric Acid.*—A case of cancrum oris has just occurred in St. Bartholomew's Hospital, under the care of Dr. Baly, which has well illustrated the usefulness of the application of the strong nitric acid. The patient was a boy, in whose left cheek the phagedenic ulceration commenced during recovery after scarlet fever. The internal use of chlorate of potass was first tried, and persisted with, in ten-grain doses, for several days, the disease, meanwhile, being unchecked. A single free application of the concentrated acid was then made to the part, and with the effect of completely arresting the morbid action. The induration of the surrounding

part has since gradually subsided, and the sore is now almost healed. The case, although not one of the most acute class, was yet of a character sufficiently alarming.—*Ibid.*

46. *Nitric Acid in Phagedæna.*—For a considerable period past, a mild form of hospital phagedæna has been prevalent in the various London institutions. In very rare cases have fatal results ensued, and in the majority, beyond the local condition, no urgent symptoms have developed themselves. It has generally attacked superficial wounds, whether those resulting from accidents or operations, and, in a few instances, even stumps have been affected with it. The treatment which has certainly been found of most general benefit has been the application of pure nitric acid. On board the Dreadnought hospital ship, where it has prevailed severely, the acid has, we believe, been found most successful. In the London Hospital, Mr. Ward informs us, that he has carefully watched upwards of twenty cases, and in no single one of these has the gangrene spread after the application of the escharotic. In St. Mary's Hospital, some remarkable cures have also been effected by its aid. In a case under the care of Mr. Coulson, a man, in good health, admitted on account of a laceration on the dorsum of the right foot, had the wound so severely affected by phagedæna that amputation was seriously proposed. The nitric acid was freely applied, by soaking a piece of lint, and then pressing it into the sore; it was allowed to remain seven hours, and completely achieved its intention. The gangrene ceased to spread, the sore took on healthy action, and rapidly cicatrized. Neither in this nor in any one of several other cases in which the dorsum of the foot was the affected part did any sloughing of the tendons follow the use of the acid. It appears to be in cases of simple phagedæna that the local use of nitric acid is most to be depended upon, since those of syphilitic origin not unfrequently resist its influence. A case of the latter character, occurring in a young and seemingly healthy woman, was lately under the care of Mr. Stanley, in St. Bartholomew's Hospital, and, in spite of all measures, ultimately terminated in death. Nitric acid had more than once been freely applied, and opium, cinchona, chlorate of potash, &c. had been fairly tried. We mentioned briefly, at page 264 of this Journal for Sept. 9, 1854, a case of cancrum oris, under the care of Dr. Baly, in which the acid arrested the phagedænic action after the internal use of large doses of chlorate of potash had quite failed to do so. In the Middlesex Hospital, the favourite treatment for phagedæna is by opium pushed freely until its effects become apparent. Some very successful cases have been cured by this plan. It is observed, that there seems scarcely any limit to the quantity which may be borne, and that, generally, before any narcotic effects become apparent, the sore takes on a healthy action. Now and then the acid succeeds admirably in syphilitic cases; as a rule, however, they seem more amenable to constitutional than local measures. On this account, we but seldom see a simple trial either of the acid or any other one remedy alone. The following case, in which the only change of treatment to which the result could be referred was the application of the acid, is worthy of mention. A prostitute, aged 26, was admitted on Feb. 1, 1853, into St. Thomas's Hospital, under the care of Mr. Le Gros Clarke. She was suffering from a large and deep phagedænic sore on the left labium. The treatment consisted in the use of wine in liberal quantities, and of a draught containing full doses of ammonia and bark. A morphia pill was given each night, and the nitric acid lotion was applied to the sore. This plan was persisted with for three days, during which the sore slowly spread. On the fourth day, the phagedænic action had much increased, and it was determined no longer to delay the use of the strong acid. The woman was very ill, feeble, and without any appetite. By means of a piece of lint, the fuming nitric acid was freely applied to the whole sore. Only a few minutes' pain was complained of. On the following day, the sore presented an almost clean surface, and it afterwards rapidly cicatrized. Exactly the same general treatment had been persisted with throughout. This case is an example of what is very often observed, viz. that the pain produced is but very transitory, and much less severe than it is commonly esteemed.

The most convenient mode of applying nitric acid is by means of a glass brush; in the absence of which, however, a glass rod succeeds very well. To the use of wood or of lint there is at least a theoretic objection, on account of their permitting the charring effects of the fluid.—*Med. Times and Gaz.* October 28, 1854.

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47. *Absorption Treatment of Bubo.*—Some cases of suppurated bubo, which have recently been treated by Mr. Hilton, in Guy's Hospital, have well illustrated the efficiency of counter-irritation, as a means of procuring the absorption of pus. The application which Mr. Hilton employs is a solution of nitrate of silver of the strength of one drachm to two of water, with the addition of about three drops of the strong nitric acid. This is painted freely into the skin over the inflamed gland. Its effect is to produce great soreness of the skin and much pain, which is usually followed by rapid diminution in the size of the enlargement. Mr. Hilton informs us, that he has long used the remedy, and now considers the cases of bubo, which require opening, as quite exceptions to the rule. In several, in order to afford proof that the fluid absorbed was really pus, it was examined by a grooved needle, prior to the treatment, the puncture being at once reclosed. Even in those instances in which ultimately it becomes necessary to make an external opening, great benefit in the limitation of the suppurative action, and the absorption of the surrounding hardness, results from the counter-irritant treatment. The plan is applicable to many other diseases besides glandular abscesses; in fact, to almost all in which the absorption of recent inflammatory deposit, either purulent or fibrinous, is desired. We reported a case in the *Medical Times and Gazette* for Dec. 17, 1853, p. 629, in which it had been effectually used to remove an induration of tissues around the femoral artery, which had so obstructed the vessel as to cause a tubular dilatation. Superficial abscess in the cellular tissue may also be so treated. Chronic non-suppurated glandular enlargements and swellings of one or other lobes of the thyroid gland may also be made to diminish by it. It is probable that the nitrate of silver has no particular virtues beyond those of a severe counter-irritant, and might, were it wished, be substituted by other remedies; as, for instance, a caustic solution of iodine. It is, however, a very convenient, manageable, and efficient means, which there is no motive for wishing changed. The substitution of nitric ether as a menstruum, instead of water, would be a great improvement. (See *Medical Times and Gazette* for Oct. 14, 1854, p. 394.)—*Med. Times and Gaz.* Oct. 28, 1854.

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48. *Treatment of Gonorrhœa.* By Wm. Acton, Esq.—It is to be regretted, but still the conscientious surgeon is obliged to admit the fact, that notwithstanding all the improvements that have taken place in the treatment of diseases of the urinary and generative system during the last few years, little progress has been made in the cure of gonorrhœa. It is true that we have in this the nineteenth century a better knowledge than we before possessed of the pathology of the disease, and we have expunged many a vulgar error which was entertained about the complaint; but as to the discovery of a certain rapid cure, we are as far distant from it as ever, and perhaps it is only with a reputation such as Ricord's that a teacher dare make the acknowledgment that, after performing various experiments during a long course of years, no certain plan of cure has been discovered. This, however, is not so surprising to the old practitioner as it is to the tyro, who does not consider the situation the discharge comes from, the frequency with which the disease occurs, the liability of relapse, together with the little attention a patient will pay to the complaint; but, above all, that the urine, loaded with all sorts of stimulating substances, must necessarily pass over the inflamed surfaces many times in the twenty-four hours; and it would almost appear as if gonorrhœa would ever continue to be the *opprobrium medicorum* and the pest of the patient, notwithstanding all the science bestowed upon it. In saying this, however, I would not be understood to mean that gonorrhœa should last an indefinite length of time; far from it, for in 19-20ths of the cases the affection is to be cured readily and easily in persons who will take ordinary precautions; but every now and then cases occur which linger on many

weeks, and yield at last to change of air or abstinence from all treatment, the disease apparently wearing itself out.

During the last few years, M. Ricord has been gradually relinquishing the employment of caustic injections in the treatment of this complaint. He still thinks that in many cases this is the treatment attended with the most certain success; but every now and then instances occur in which the pain is very severe, the inflammation runs high, and then, instead of a cure being obtained, the disease relapses into a chronic state, which resists all our means of cure; in addition to this, the patient requires a great deal of watching, more than a surgeon in full practice is able to devote to each individual; so that, as I mentioned above, Ricord now employs caustic injections very rarely, and prefers prescribing capsules, and the following injection: Sulphate of zinc, acetate of lead, of each fifteen grains; tincture of catechu, one drachm; Sydenham's laudanum, one drachm; rose water, six ounces and a half. My own practice in London fully bears out this experience, and it is only in old rebellious cases of gleet, in spermatorrhœa, and in chronic affections of the bladder, that I now employ nitrate of silver injections; but in these cases the judicious employment of caustic is of the greatest value, and will at once effect a cure when all other means fail; still, even in these cases, the remedy must be employed with care, otherwise ill consequences will arise.

In the subacute stages of gonorrhœa, when there is no scalding, but a good deal of discharge, I have found the solution of lead answer best in London, together with copaiba capsules, not taken in large numbers, but at repeated intervals, so as to charge the urine constantly with the essential oil; but I still (as in the last edition of my book) continue to insist that gonorrhœa will rarely be rapidly cured, unless the surgeon takes the precaution to show a patient how to use his syringe, and see that the instrument be properly made. Having insisted elsewhere in great length on these points, I do not think it necessary here to revert to them; but to these petty matters I attribute much of my success, and not to any specific treatment. It is, however, worthy of note, that the time of year and the damp weather have a great influence in retarding a cure.—*Lancet*, Oct. 14, 1854.

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49. *Carpet Tack in the Trachea for Nineteen Days; Expelled by Nature.*—JAMES CRAWFORD, M. D., Professor in McGill College, reports (*The Medical Chronicle*, Oct. 1854) the following interesting case:—

M. G., a girl of ten years of age, while sitting beside her mother, who was occupied in quilting, took one of the tacks her mother was using, and put it into her mouth, and upon a sudden inspiration the tack disappeared into the trachea. After the alarm was over, the child complained of a pain or prickling sensation in the throat, accompanied by a short frequent cough, occurring in paroxysms. The parents, in a great measure, overlooked the accident for ten days, when they brought her to me. At that time there was no very marked symptom; the cough was trifling and short, without any expectoration. There was a slight mucous rale in the trachea, and some slight pain or soreness about the region of the glottis, but probably from fear; the child did not desire to make any complaint, or allow any examination of the throat. The lungs did not afford any indication of an abnormal condition, and the voice remained unaffected. Failing in detecting any foreign body, I fancied the painful sensation might arise from the scratching of the nail, in passing. I sent the child to my friend Dr. Campbell, to make a further examination, as I knew he had a probang of a new construction. The party, however, returned back to me, saying that nothing could be discovered. I then ordered an emetic, and directed that the child should be brought to me in a few days. As she did not, however, appear to suffer, and she went about and amused herself as usual, the parents did not pay much attention to her. On the morning of the 19th day, she coughed up the tack, and took it to her father, who brought it to show me. It was an ordinary cut tack, of three-fourths of an inch long, very sharp pointed, and somewhat rusted. From that time the cough and irritation ceased.

50. *Expulsion of a Coin from the Larynx by an act of Coughing.*—Dr. T. G. Geoghegan relates (*Dublin Medical Press*, Oct. 11, 1854) the following case of this description, and offers some observations on the proper course to be pursued in such accidents, which are worthy of attention.

A healthy lad of 17, admitted into the City of Dublin Hospital, stated that during a quarrel with one of his fellow-servants, having at the time in his mouth a sixpence and a fourpence, he spat out the former coin, while the latter disappeared. He was immediately seized with cough, of such violence as to produce epistaxis, and great difficulty of breathing; both of which, however, speedily subsided. He was seen two hours after the accident, and was then aware that the coin had entered the windpipe. While at rest, he suffered no inconvenience, and the breathing was quite unaffected. When desired to cough while in the erect or sitting position, a peculiar valvular or obscure rattling sound was distinctly audible in the larynx. The stethoscope placed on the organ did not render this at all more perceptible, nor could it be elicited (save on one occasion) while the patient was in the horizontal posture. It somewhat resembled that which attends an unsuccessful effort to expel a mass of tough mucus. Respiration was of equal intensity under both clavicles. During the night, he had a few fits of coughing. On the next day, the symptoms were in all respects the same.

In considering the mode in which the case should be dealt with, and reflecting on the uncertain issue of the natural expulsory efforts, the possibility of their leading to sudden suffocation of the patient, by impaction of the coin in the glottis, or the risk of the body being transferred to the bronchus, it appeared to me inexpedient to temporize.

The physical signs seemed to indicate pretty clearly that the coin was seated in the larynx.[1]

I proposed, therefore, to make a suitable opening in the crico-thyroid space, and, should the foreign body be readily seized, to extract it by the forceps. In the event of this manœuvre not proving successful, the expedient of inversion of the body, aided, if necessary, by percussion, was to be then resorted to. By this mode of proceeding, I had hoped at once to gain more direct access to the offending substance, and to save the patient the more dangerous operation of tracheotomy. In order to give full effect to this method, it would be necessary, before opening the crico-thyroid ligament, to effectually restrain any hemorrhage arising from the laryngeal branch of the superior thyroid artery, which, though small, often bleeds rather freely, and also to have the patient carefully maintained, as far as possible, in the horizontal posture, during the employment of the forceps, and subsequently, until inversion had been practised, if necessary.

Although the course above proposed received the assent of my colleagues and other surgical friends, it by no means satisfied the patient, who, notwithstanding suitable explanations, withdrew from the hospital disconcerted. He returned to his employment, and on the morning of the third day, fifty hours after the accident, while stooping in the act of washing a carriage, he was seized with a fit of coughing, and expelled the coin. This he returned to exhibit with a certain amount of triumph.[1]

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[1] Morbid growths in the larynx, when pendulous, give rise, in a modified degree, to the symptoms of foreign body. An interesting example lately presented itself in an hospital patient of mine, who died of another disease. He had been subject for some years to attacks of irritative cough; uneasiness referred to the situation of the thyroid cartilage, and, latterly, almost complete aphonia. I found, on dissection, a soft, lobulated growth of rather considerable size, translucent, of a light gray colour, and strikingly similar to nasal polypus. It was loosely attached by a narrow pedicle to the middle of the lower chorda vocalis of the right side. A similar, but smaller and sessile, tumour was observed on the left upper one. I believe that growths of this nature are rather rare. If the disease admitted of an accurate diagnosis, it might possibly, in some cases, fall within the range of operative surgery. When the morbid growth becomes detached (as in the case of tuberculous mass, which I have recorded in the *Medical Press*, vol. xi. p. 103), the symptoms will of course be essentially the same as those observed in cases of foreign body introduced from without.

The instance now recorded may be considered as one of those less common cases in which a foreign body has been expelled from the air-passages by natural efforts. As such a result, however, must be altogether fortuitous, it cannot, according to my judgment, be construed as affording a precedent for withholding an operation on similar occasions. This view will, I apprehend, be readily conceded by those who are conversant with such accidents; for there is perhaps no occurrence in surgical experience, the results of which are more varied, and sometimes more unexpectedly fatal, than that in question. In some instances, where the body, in entering, has been caught in the glottis, and possesses a suitable figure, complete obstruction and immediate death result; in others, having passed the aperture, it may become engaged in the ventricle of the larynx, or the crico-thyroid hollow; or, finally, it may descend into the bronchus. In the latter case, as the interbronchial crest deviates to the left of the median plane, the foreign substance will generally occupy the right bronchus. Should it be a light substance, it may be dislodged from thence by the unassisted efforts of coughing, and be either fortunately expelled, or kill the patient by becoming impacted in the glottis; or, lastly, it may become entangled in the interior of the larynx. Should it, on the contrary, be a heavy substance, such as a coin or bullet, it may become lodged for an indefinite period in the bronchus, and having produced symptoms more or less grave and persistent, either terminate the patient's existence by suppuration or gangrene of the lung, or (as occurs much more rarely) be expelled by cough when assisted by a favourable position. Of the latter happy result, a case has been lately recorded, in which a pistol bullet, under the conditions just assigned, was coughed up from the bronchus after forty days.

I think it, on the whole, very probable that, in the present case, the coin was lodged in the *ventricle* of the larynx. Its size and figure, taken in connection with the mildness of the symptoms, and the peculiar effect of change of posture on the sound of the cough, appears to support this view. Had the body not become thus engaged, it is likely that, in a subject of the patient's age, it would, as in Mr. Brunel's case, have descended to the bronchus, especially as the diameter of a fourpence is to that of a half sovereign but as five to six. When the foreign body occupies the situation just described, it does not, however, the less endanger life, nor is it the less subject to sudden, disastrous, and unexpected changes of position. In many cases (perhaps in most), no precise evidence can be obtained of its exact situation.

Auscultation and percussion will afford valuable aid in some instances; as, for example, where a body of extended surface or considerable size occupies the bronchus in a position favourable to nearly complete obstruction of that tube. In many other cases, however, where the body occupies the larynx or trachea, and, as so often happens, is enveloped in mucus, or where it is of small volume, little assistance can be expected from the stethoscope; still, even its *negative* indications should be invariably sought for. In estimating the diagnosis of foreign bodies in the windpipe, I may be permitted, lastly, to remark that sufficient stress, as it appears to me, has not been laid on the fact that, although the pulmonary or laryngeal symptoms are in themselves often equivocal, they have suddenly attacked an individual who had, immediately before, been engaged in some unguarded respiratory movement, and who had some foreign substance at the time in the mouth, and that although at first urgent, these symptoms often subside, to recur again with greater or less intensity.

With regard to the treatment which I had proposed to myself, it will be observed that, in its essential features, it would have been the one adopted by that distinguished surgeon and physiologist, Sir B. Brodie, in the case of Mr. Brunel. From the indications afforded by the symptoms, it appeared to me, however, that, in the present case, it would have been more feasible to have opened the crico-thyroid space (by *full* crucial incision and reflection of the flaps) than to have resorted to tracheotomy; for, considering the coin to be situated either in the ventricle or in some other part of the larynx, I judged that it would have been thus more directly accessible, in the event of its lying in a favourable position for immediate extraction.

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[1] By a singular coincidence, I removed, not long afterwards, from the rectum, by the forceps, a fourpence, which had been swallowed, and arrested by a narrow stricture of that organ. It was blackened by the sulphur of the intestinal secretions. The coin from the larynx had, of course, undergone no such alteration.

In a case in which I removed a dog's tooth from the larynx of a girl of seven years, by tracheotomy, I found it impossible to reach the body with the curved forceps, until, fearing injury to the mucous membrane of the larynx, I prolonged the incision, not only through the cricoid cartilage, but also through part of the crico-thyroid ligament. Mr. Porter, who was present, concurred entirely in the propriety of this proceeding, which, I had the satisfaction to find, left the functions of the larynx quite unimpaired. As to the practicability of removing a body of the diameter of a fourpenny piece through the crico-thyroid space, this will be sufficiently apparent when I state that I have found the transverse diameter of that space, in the adult, to be seven-eighths of an inch, and double that of its vertical. Now, as a fourpence has a diameter of five-eighths of an inch only, ample space would be thus afforded even for a larger body, especially when the elasticity of the parts is considered.[1]

I do not perceive any valid reason, anatomical or physiological, why the tube should not be divided when necessary (as in the case just mentioned) through the cricoid, or even through the *thyroid* cartilage; provided always, that *the median line* be strictly observed, and I feel satisfied that any speculative danger on that score is far outweighed by the actual mischief inflicted on the delicate and important mechanism of the larynx, by repeated ineffectual efforts to grasp the body from the *trachea.* The unossified cricoid cartilage permits of considerable divarication of its cut edges without any force; and in the event of the foreign body having passed into the bronchus, the space thus gained might be increased without danger by the removal of a thin slice from the margin, as often done in the trachea, and by prolonging the incision into the upper rings of the latter. A suitable slender forceps might be then readily introduced, while some of the dangers and inconveniences of tracheotomy would be thus avoided. I have found in the living subject, as above related, that the division of the isthmus of the thyroid body is not necessarily accompanied by any hemorrhage, nor is the latter likely to occur to any important extent, even in the adult, when the structure of the gland is not diseased. The operation just described, which, I may observe, has been proposed, and I believe performed, by some of the older French surgeons, would thus have the advantage of rendering *both* the larynx and bronchus accessible in those numerous cases in which doubt exists as to the precise situation of the foreign body.

With regard to the operation of inverting the patient's body, viewed in connection with an artificial opening in the windpipe, this well-considered manœuvre, as is well known, was crowned with success in Mr. Brunel's case, and happily illustrates the importance of that union of physiological and surgical science, of which Sir B. Brodie's career affords so distinguished an example. Had it been necessary to have resorted to this procedure in the case which forms the subject of the present observations, the question might naturally have arisen, Whether the proximity of the proposed artificial opening to the glottis, would have exerted any influence in preventing that quiescent condition of the muscles of the latter, which it is the intention of the operation to secure? Further: Whether inversion should *precede the* employment of the forceps, or be resorted to only in the event of the failure of the latter? And lastly, where the former plan has been decided on, the inquiry would suggest itself, whether any, and what interval should elapse bewteen laryngotomy and inversion? As respects the attempt to grasp the foreign body by the forceps, should it be found tangible from the wound, I apprehend few surgeons would be disposed to forego the attempt, in favour of a preliminary inversion of the patient, which, if unsuccessful, might lead to a transference of the substance to the bronchus, &c. These are questions which, within the limited number of facts at present at our disposal, cannot be answered with precision. In Mr. Brunel's case, sixteen days, I believe, elapsed between the operation and the subsequent inversion, and it seems, on general grounds, not unlikely (at least where a previous attempt to grasp the body has failed) that *some* interval should elapse, in order to reassure the laryngeal muscles, and to allow the glottis to acquire the passiveness essential to the success of the manœuvre. In confirmation of this view, I think it will be found that in most cases where food has passed into the larynx, through a glottis paralyzed by tracheotomy, this result has not taken place very shortly after the operation.

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[1] I find the diameter of a half sovereign to be six-eighths of an inch.

When a coin has entered the larynx, it has been proposed by Dessault, and also by later observers, to introduce a gum-elastic catheter into the glottis, in the hope that, while air would be thus applied, the foreign body would glide edgewise along the instrument when the patient was inverted, and that hence an artificial opening might be dispensed with. Such a procedure, however, appears, on obvious grounds, to be not very feasible.

Although, in the foregoing observations, I have, in common with most surgeons, advocated the propriety of not trusting to the uncertain issue of natural efforts for the expulsion of the offending substance, still, no one who has witnessed many such cases can be insensible to the dangers which invest operations on the air-tube. Some of these arise, at times, most unexpectedly, as, for instance, spasm of the glottis at remote periods, after removal of the canula, when all is thought secure. But, on the other hand, circumstances occasionally occur, which must not be hastily charged on the operation. Thus, in a case which I have already recorded, the impaction of a mass of viscid mucus in the bronchial bifurcation caused sudden death many days after tracheotomy had been resorted to. Again, congestion of the brain sometimes takes place as the result of the violent respiratory struggles which succeed the accident, and may terminate life, *after the operation*, with convulsive or comatose symptoms, analogous to those observed in the secondary asphyxia of submersion.

OPHTHALMOLOGY.

51. *Successful Operation for Congenital Cataract.*—[The following case, communicated to the Royal Medical and Chirurgical Society (Nov. 14, 1854), by G. Critchett, Esq., is interesting, and the discussion which followed, respecting the use of chloroform in operations on the eye, is particularly so. The statement of Mr. Thomson, that he had noticed deterioration of sight constantly to follow operations for strabismus, is contradicted by the experience of, we believe, every surgeon, and is certainly the reverse of what we have ourselves observed.]

Jane S., aged twenty-two, was brought to the Royal Ophthalmic Hospital in the spring of 1849, suffering from cataract in both eyes, with slight internal strabismus, and considerable involuntary rolling and oscillation of the globes from side to side. The corneæ were bright; the anterior chambers large; pupils small and irregular, and filled with an opaque white substance; perception of light was good. She had frequently been operated on before. Mr. Critchett first succeeded in detaching the adherent iridis from the capsules by the usual needle operation, the puncture being made through the cornea. By a second proceeding, he made a small opening through the scleroticæ, and dragged the capsule from the sphere of vision by means of a hook in one eye, and fine forceps in the other. Then to remedy the oscillatory movement, which prevented the impression of any object distinctly upon the retina, he divided, first the internal and then the external recti muscles. The sight is now improving, and by education she is slowly beginning to make some valuable use of her newly-acquired sense.

Mr. Fergusson observed that it was not often that papers referring to operations on the eye came before the Society, and he therefore would put a ques-